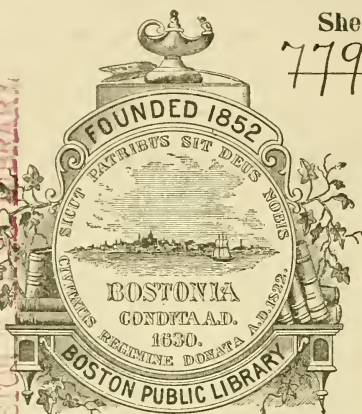


HISTORY OF THE
CHOLERA CONTROVERSY

SIR GEORGE JOHNSON M.D.

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HISTORY OF THE CHOLERA CONTROVERSY

*WITH DIRECTIONS FOR THE TREATMENT
OF THE DISEASE*

BY

SIR GEORGE JOHNSON, M.D.LOND., F.R.C.P., F.R.S.

FELLOW OF KING'S COLLEGE, LONDON; EMERITUS PROFESSOR OF
CLINICAL MEDICINE; CONSULTING PHYSICIAN TO
KING'S COLLEGE HOSPITAL
PHYSICIAN EXTRAORDINARY TO HER MAJESTY THE QUEEN



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P R E F A C E.

AN excellent authority has recently declared that "It is now admitted nearly on all sides" that the rational, scientific, and successful treatment of cholera has been proved, especially during the recent epidemic in Germany, Russia, and Italy, "to be mainly eliminative and antiseptic." (See p. 55.)

The subject is one of high scientific interest, and of deep practical importance, and I have undertaken to give a brief history of the controversies relating to the pathology and treatment of this terrible disease which have been carried on during the last forty years and more. I have abundant materials for such a history ; for since the commencement of the controversy, during the epidemic of 1854, I have collected and arranged in a series of scrap-books the most important of the published papers, reviews and correspondence, together with many private letters, which this prolific discussion has called forth. Some of the most interesting and instructive of these documents I have reproduced in the following pages.

It will, I trust, be admitted by all unprejudiced readers of this history that the theory which attributed choleraic collapse mainly to the loss of water by the stomach and bowels, and suggested the repressive treatment by opium—this theory, plausible as it appeared, is an erroneous theory, based on an incomplete observation of the symptoms of the disease, and constructed by men who had but an imperfect knowledge of the physiology of the circulation of the blood.

In fact, when this theory was first promulgated, more than sixty years ago, the most accomplished physiologists of that day had no knowledge of the structure and functions of the muscular-walled arterioles. It was not until some years later that the important discovery was made that the resisting force of the contracting arterioles is more than equal to the propelling power of the heart; so that by the conjoint action of these Lilliputian stop-cocks the circulation may be speedily and completely arrested. This great physiological discovery is the key to the pathology of the collapse stage of cholera.

The most melancholy part of this sad story is that the men who accepted the erroneous theory and carried it into practice failed to see the disastrous results of the astringent treatment which it suggested. This inability to recognise the calamitous results of a popular but erroneous theory has been

referred to by H. T. Buckle, in his "History of Civilisation in Europe." He says: "There is no well-attested case on record of any theory having been abandoned because it produced injurious results. As long as a theory is believed, men will ascribe its evil consequences to any cause except the right one. And a theory that is once believed will always be believed until there is some change of knowledge which shakes its foundation. Every practical change may, by careful analysis, be shown to depend, in the first instance, on some change of speculative opinions." I have good reason to believe that at the present time few, if any, men of influence accept the exploded "dehydration" theory of collapse, or adopt the injurious practice which it dictated.

As it would be impossible to estimate correctly the number of victims that have been sacrificed to this "idol" of the human mind, so it would be impossible to over-estimate the responsibility of any one who propounds a theory which is calculated to greatly influence medical practice. All through this prolonged and, too often, acrimonious controversy, I have had a deep and almost painful consciousness of my own responsibility; but I have, from time to time, been assured by men most competent to form a sound judgment that, in their opinion, the theory of choleraic collapse which, after a most careful inquiry, I first published in 1855, is in accordance

with all the known facts of the disease, and with the established principles of physiology.

Sir Thomas Watson, while President of the Royal College of Physicians in 1866, referred to it as "a sound as well as a most ingenious and important theory. In truth, it derives strong confirmation from the fact that it unlocks, like the true key, the whole of the pathological intricacies of the disease." This is strong testimony from a great and wise man, but that which, to my mind, is still more satisfactory is the evidence which has been gradually accumulating of the beneficial results of the evacuant treatment of cholera, especially in its early stages. What can be more conclusive than the fact, referred to in detail hereafter (p. 29), that in Liverpool* during the epidemic of 1866, out of several thousand cases of choleraic diarrhœa treated by laxatives not one passed into collapse or required removal to the hospital? "In every case relief was afforded 'quickly, safely, and pleasantly.'"

No such evidence as this ever has been or can be given in support of the astringent treatment of the disease.

Although this essay is mainly a history of past controversies, and not a complete treatise on the pathology and treatment of cholera, it will, I think,

* "On the Treatment of Cholera and Epidemic Diarrhœa," by Dr. M'Cloy and Dr. Robertson: *Med. Chir. Trans.*, vol. 1.

be found to give a clear sketch of the entire subject ; but I must refer those who wish to read a full and complete discussion of the pathology of the disease to the chapter on “ Epidemic Cholera ” in my *Medical Lectures and Essays*.

In the concluding section of this treatise I have given detailed directions for the prevention and treatment of diarrhœa and cholera. It will be seen that the method of treatment, which ample experience has proved to be highly successful, is entirely consistent with what is now acknowledged to be the true pathology of the disease.

11 SAVILE ROW,

Nov. 29th, 1895.

HISTORY OF THE CHOLERA CONTROVERSY

I. My First Experience of Cholera in 1849.

I PROPOSE in this treatise to give a brief history of the various theories with regard to the pathology of Cholera, and the different modes of practice, suggested by those theories, which have been adopted by the profession during the last forty years.

During my student days I was taught that the worst symptoms of cholera were due to the drain of water from the blood and tissues, and that the main object of treatment in the early stage of the disease was to arrest the gastro-intestinal discharges by opium.

I first saw cholera during the epidemic of 1849. I was then resident medical tutor in King's College, and I was asked to prescribe for the wife of one of the porters of the College. She was about forty years of age, and had been in good health until she was seized with vomiting, purging and cramps.

She had a warm skin and a good pulse, and I looked upon the case as a mild one. In accordance with the teaching that I had received, I gave her five grains of Dover's powder every hour until three doses had been taken. When I saw her again, in about three hours from the time of my first visit, I found to my dismay that although the vomiting, purging and cramps had ceased, she was in full collapse, and she died in a few hours.

I saw a similar result follow the like treatment by other physicians at the hospital. I was horrified to see that in case after case the speedy arrest of diarrhœa by opium was directly followed, not by recovery, as in accordance with the then generally accepted theory it ought to be, but by rapidly fatal collapse. These facts convinced me that the theory was wrong, and the treatment based upon it injurious.

II. My Successful Trial of the Eliminative Treatment in 1854, and the opposition it excited.

In 1854 came another epidemic of cholera. I was then the junior assistant physician of King's College Hospital, and during almost the entire period of the epidemic I was left in entire charge of the medical wards.

It was an eventful time, and I entered upon the work with fear and trembling. I remembered that

in 1849 about three-fourths of the cases of cholera in our hospital treated by brandy and opium were fatal, while the same proportion of about an equal number treated by copious draughts of salt and water recovered.

So far as I could learn from reading, two of the most successful methods of treatment, judging by the results apart from theories, were that of Dr. Ayre, by frequent small doses of calomel, and that of Dr. Stevens, by repeated doses of salines. That which was common to these two methods was that they tended to encourage and not to repress the discharges from the bowels. Then, acting upon a provisional hypothesis, that the discharges are the means by which the cholera poison is ejected from the system, I determined to try the effect of the quickly acting evacuant castor oil, and the results were surprisingly favourable and encouraging.

The first seven cases of choleraic collapse that came under my care were treated by more frequent doses of castor oil than I afterwards found to be necessary or desirable ; but they all recovered, the collapse passing off while the discharges continued. The inference was obvious : the discharges from the stomach and intestines are the means by which the cholera poison is ejected and the patient is restored to health.

On the 6th of September 1854 I addressed to the editor of the *Medical Times and Gazette* a letter, in which I stated that I had treated fifteen cases of choleraic collapse by castor oil with only three deaths,

one of which was fatal within half an hour after admission. I further stated that hundreds of cases of choleraic diarrhœa, including several nurses and one pupil in attendance in the cholera wards, had been quickly cured by one or two doses of castor oil, and that not one had passed into collapse.

The publication of this letter led to such an outbreak of censure and opposition as probably no other member of the profession has ever been assailed with. The most atrocious attack upon me by any medical journal was contained in a leading article in the *Association Medical Journal*. The editor was Dr. (afterwards Sir John) Rose Cormack, and if he did not write the article, he was responsible for its publication.

The writer, after commenting on what he considered the unreasonableness of my theory and the danger of my practice, proceeded as follows: "With Dr. Johnson's opinions we have now done. It is evident that the author has gone astray from the right paths, and has subjected himself to the imputation of belonging to a class by whom (to use the language of Bishop Butler) arguments are often wanted for some accidental purpose, but proof as such is what they never want for their own satisfaction of mind or conduct in life."

This sapient writer was an open enemy, who might have been prosecuted for a malicious libel, but most of my best friends believed my theory and practice to be wrong.

Dr. Robert Ferguson said to Mr. Charles Hawkins, as I heard many years afterwards in a conversation with Mr. Hawkins at the Athenæum Club, "What a pity it is that Johnson has taken up this notion about cholera. It will be absolute ruin to him—absolute ruin."

Dr. Budd (who was then senior physician of the hospital, and whom I afterwards succeeded as Professor of Medicine) came back from his holiday when the epidemic was over, and gave a clinical lecture, in which he made no mention of me or the work which had been going on in his absence, but told his class that the proper treatment of choleraic diarrhoea was to arrest the discharges by opium. Some of his pupils, who had watched my practice in the cholera wards, reported to me his words, but felt that they knew more of the subject than the Professor.

Dr. Todd, who was one of my kindest friends and benefactors, said to me when the epidemic was over: "I believe that in the main you are right about cholera, but you will not convince the profession that you are, and in attempting to convince them you will do yourself great harm. I advise you to publish no more on the subject."

This was kind and friendly advice, but I told Dr. Todd that I could not act upon it, and that I had determined to publish the notes of all my cases, together with such information relating to the pathology of cholera as I might be able to obtain.

III. Results of my Study of Cholera Literature.

For a period of about six months I devoted almost the whole of my time to the careful reading and analysis of books and reports on cholera, most of which were published by men who had had a large experience of the disease in India. At that time, while I was under a cloud of misrepresentation, with the nickname of "Castor Oil Johnson," my reading was but little interrupted by practice, and if I had not gained a certain reputation in connection with kidney disease, my friend Dr. Ferguson's gloomy prophecy might have been fulfilled. It was a time of great interest to me, for I was daily increasing my knowledge of the disease, and from time to time lighting upon facts which were obviously inconsistent with the theory which assumes that the chief cause of collapse is the drain of water from the blood. The majority of the Indian practitioners were agreed that, while the most deadly cases of the disease were those in which the patient falls rapidly into collapse, with little or no discharges, a complete arrest of the discharges during the stage of collapse is a sign of fatal import. Death may take place without discharge ; recovery never.

Then it is very remarkable that no Indian practitioner, writing before cholera became epidemic in Europe in 1831-2, appears to have given opium for the purpose of arresting the discharges. Opium was

often given to relieve the painful cramps, but at the same time purgatives were employed to remove the offensive morbid secretions.

Dr. Edmund Parkes* was the first to publish the fact that when the inspection of the body is made soon after death during the stage of collapse, the right side of the heart, the pulmonary artery, and the large systemic veins are distended with blood, while the left side of the heart and the systemic arteries are nearly or quite empty. These facts indicate, as Dr. Parkes said, that during the collapse stage there is an impeded flow of blood through the lungs. Dr. Parkes was unable to explain this impediment, for the simple reason that his book was published four years before the earliest of M. Claude Bernard's researches on the vaso-motor nervous system were made known to the world.

IV. Publication of "Epidemic Diarrhœa and Cholera," with my Theory of Collapse, in 1855.

When, in 1855, I published my book "On Epidemic Diarrhœa and Cholera," the function of the vaso-motor nerves and of the arterioles had been ascertained, and I was enabled to suggest that the impeded circulation through the lungs during choleraic collapse is a result of contraction of the

* "Researches on the Pathology and Treatment of the Asiatic or Algide Cholera." 1847.

muscular arterioles excited by the poisoned blood. This action of the vaso-constrictors of the lungs explains all the most striking phenomena of choleraic collapse. It explains the emptiness of the systemic arteries, with extinction of the pulse at the wrist, the cadaverous shrinking of the features and sinking of the eye-balls, also the lividity of the surface, consequent on the venous fulness. The stream of blood through the pulmonary capillaries being much decreased, the supply of oxygen is proportionally reduced in quantity. Hence during the stage of collapse there is defective oxygenation of the blood and the tissues, with resulting lowering of the surface-temperature, diminished exhalation of carbonic acid, and almost complete suppression of bile and urine; carbonic acid and the chief constituents of bile and urine being joint products of oxidation. That this is the true explanation of the suppression of bile and urine is rendered the more probable from the curious fact that when a nursing mother becomes the subject of choleraic collapse the secretion of milk, though poor in quality, is but little diminished in quantity. Now the chief constituents of milk—casein, sugar, fat and water—are not oxidised products.

The impeded circulation through the lungs is the *cause* and not a *result* of the blood thickening in the collapse stage of cholera. So long as the circulation is free, diarrhœa, no matter how copious, does not cause thickening of the blood. The loss

of water by the bowels is replaced by absorption of water from the muscles and other soft tissues, which contain water in the proportion of four-fifths of their weight; but when the vaso-constrictors of the lungs impede the onward passage of the blood, the entire systemic venous system is engorged, and water escapes from the blood into the partially dehydrated soft tissues, while venous absorption from the stomach and bowels is impeded. Defective oxidation, consequent on the scanty stream of oxygen-carrying blood through the lungs, contributes to the blood thickening by lessening the disintegration of the solid constituents, which consequently accumulate in the blood. Dr. Dundas Thompson found the blood of a patient suffering from an affection of the bronchial mucous membrane thicker than any cholera blood that he had ever examined. (*Med. Chir. Trans.* vol. xxxiii.)

The theory that choleraic collapse is the result of extreme constriction of the pulmonary arterioles explains, in the worst class of cases, the sudden onset of collapse, and in other cases the equally rapid recovery. Twining, an Indian practitioner, says: "In these instances recovery seems almost as rapid as in cases of patients who are resuscitated after suspension of animation from submersion in water." No such cases of rapid recovery are possible after extreme exhaustion by profuse discharges from the blood and bowels.

The marvellous temporary benefit that results

from the injection of a warm saline solution into the veins, which has been generally assumed to lend support to the theory that dehydration of the blood and the tissues is the main cause of collapse, receives a more probable explanation, from the relaxing effect of the warm liquid upon the constricted pulmonary arterioles. Dr. Mackintosh of Edinburgh injected the veins of 156 patients in 1832.* The temperature of the fluid injected ranged from 100 to 120 degrees Fahr., but he states that "the good effects of the injection were rapid in proportion to the heat of the solution."

Several of the Indian practitioners describe the great relief which has often been afforded by venesection during the stage of collapse. The following striking case is recorded by Sir Ranald Martin :† "On visiting my hospital in the morning the European farrier-major was reported to be dying of cholera. I found that during the night he had been drained of all the fluid portion of his blood. His appearance was surprisingly altered; his respiration was oppressed, the countenance sunk and livid, the circulation flagging in the extremities. I opened a vein in each arm, but it was long before I could obtain anything but trickling of dark treacly matter. At length the blood flowed, and by degrees the

* "Practice of Physic," 1st edition, vol. i. p. 363.

† "The Influence of Tropical Climates on European Constitutions," 6th ed. p. 349.

darkness was exchanged for more of the hue of nature. The farrier was not of robust health, but I bled him largely ; when he, whom but a moment before I thought a dying man, stood up and said, ‘Sir, you have made a new man of me.’ He is now alive and well.”

The explanation of this striking result of a practice inconsistent with the operator’s theory, is that there was a paralysing over-distension of the right side of the heart, consequent on the impeded circulation through the lungs. Venesection relieved the over-distension of the right cavities, and so increased the contractile power of their walls. The experiments of Dr. John Reid and others have proved that venesection often restores the contraction of the over-distended right cavities in asphyxiated animals.

The distension of the right side of the heart after death from choleraic collapse is precisely the same as is found after death from asphyxia, and extreme contraction of the pulmonary arterioles is the only possible explanation of the arrested circulation in both classes of cases.

The doctrine, that the essential cause of choleraic collapse is the greatly impeded circulation through the lungs, receives remarkable confirmation from the fact that several cases are recorded in which a fibrinous plug in the pulmonary artery has caused symptoms strikingly resembling cholera.*

* See the chapter on Epidemic Cholera in my “Medical Lectures and Essays.”

The most remarkable case of this kind is one published by Dr. Alfred Carpenter (*The Lancet*, Sept. 23, 1871). Dr. Carpenter remarks that "the only symptoms wanting to make it apparently a case of cholera were alvine discharges and cramps in the limbs." Great thirst and almost complete suppression of urine were prominent symptoms.

The preceding is a brief abstract of facts and reasonings, most of which are fully related in my book "On Epidemic Diarrhœa and Cholera," which was published in 1855.

That work was the result of much labour and careful thought, and I naturally looked with interest to see how it would be dealt with by the journals which are supposed to lead professional opinion.

V. Reviews of my First Book on Cholera.

The *Lancet* (June 2, 1855) began a notice occupying about a column with the following sentence: "The essential point in connection with cholera, with which the name of Dr. George Johnson was rather prominently obtruded upon the public, is by far too well known and easily defined to be lost sight of in the physiological adornments with which the author attempts to dress the question and dazzle the reader in the 300 pages now before us."

The writer probably had not read the book, or, having read it, he had not the physiological knowledge which would have enabled him to give a fair review of the work. He says: “This work is nicely written, his physiology may appear unexceptionable, and his pathology may be as sound as the school he is attached to is capable of,” but “the Committee of the Medical Council of the General Board of Health has shown in its late Report on the results of the different modes of treatment, that the procedure in question [the eliminative plan] is about the worst that has ever been promulgated.” I shall hereafter show how entirely untrustworthy is the Report to which my critic referred. (See p. 27.)

The *Medical Times and Gazette* (June 23rd, 1855) devoted four columns to a full, fair and courteous review, which showed that the writer had carefully read the book, of which he gave a clear, correct and intelligible analysis. “The book,” he says “is the production of one of the most acute and able physicians of the modern school; one whose labours in pathology entitle him to a full and fair hearing.” “Our own sentiments on the merits of the book are that the author seems always too eager to prove his case. His very profound knowledge of modern physiology and pathology seems to betray him into too elaborate and plausible chains of argument. His dexterity in explaining away whatever opposes his views is remarkable, no less

than his eagerness at seizing on any incident, however trivial, that supports them." This expression of opinion is fair enough ; but I might ask whether my eagerness to prove my case had been as great as that of my opponents to disprove it, and whether my dexterity in explaining away whatever opposed my views had been equal to the blindness of those who failed to see the significance of collapse following directly upon the arrest of the discharges by opium.

Finally the reviewer says, " We have frankly stated our objections to this very ingenious and able work, which we advise our readers to peruse carefully for themselves, with the assurance that they will find it abundantly instructive ; and even if they are not converts to the eliminative plan of treatment (for we must observe that Dr. Johnson nowhere asserts that castor oil is *the* specific), yet they will respect the author for the courage and zeal with which he enunciates doctrines which he knows to be unpopular, no less than for the extensive information which supplies him with arguments from every department of physiology and pathology."

The editor of the *Association Medical Journal* did not notice the book. Perhaps he would have found it difficult to give a fair review of the work after his statement that the author had "deviated from the right paths," &c. See *ante*, p. 4.

**VI. Publication of "Notes on Cholera" in
1866 and its Favourable Reception by
Sir Thomas Watson and other
Eminent Men.**

When my book was published in 1855, the epidemic had passed away, and with it departed the alarm and the interest it had excited; my work, therefore, attracted but little attention, and probably had very few readers.

After an interval of ten years cholera again appeared to be approaching our shores, and, in anticipation of a probable return of the disease, I published a series of papers in the *British Medical Journal*. These papers I afterwards collected, revised and published at the beginning of 1866, in a small volume entitled "Notes on Cholera." This volume excited much interest, and, perhaps on account of its smaller size, had many more readers than my previous work on the same subject, but in my judgment the most important result of its publication was that it made a convert of Sir Thomas Watson, who was at that time President of the Royal College of Physicians. I had sent him a copy of the book, and on January 24th, 1866, I received from him a letter, from which the following is an extract. "I am bound to say that I find no flaw in your reasonings. Your position is admirably well expounded and sustained, and the pathology that you adopt is rational and plausible

in the best sense of that word. It seems also to be justified by the results of your peculiar treatment of the disease, and by treatment that resembles yours." I need not say that to receive this testimony as to the reasonableness of my views from one of the wisest, most cautious and conscientious of men was very gratifying and encouraging, after the adverse criticism and censure which my advocacy of the eliminative treatment had called forth.

So great an interest did Sir Thomas Watson take in the subject that, notwithstanding the many calls upon his time, he wrote a review of the book, which was published in the *Saturday Review*, June 2, 1866.

After giving, in his own inimitable style, a lucid summary of my explanation of choleraic collapse, the reviewer went on to say: "Surely this seems a reasonable theory. It is founded on a true analogy, it is consistent with the symptoms noticed during life and with the conditions discovered after death; we may therefore legitimately regard it, until fairly refuted, as a sound, as well as a most ingenious and important theory. In truth, it derives strong confirmation from the fact that it unlocks, like the true key, the whole of the pathological intricacies of the disease."

Sir Thomas Watson then concluded his article in the following terms, which, by the favour of the author, I was permitted to copy from his manuscript: "We need not abstain from expressing our

belief that the true pathology of this awful disease, the sequence and relation of its various phenomena, have now for the first time been determined and made known; and we congratulate the eminent physician who has done the world this great service, on the firmness of purpose with which he has withstood the force of ridicule in high places, and upon the perseverance and sagacity which have enabled him to solve so neatly and thoroughly a very complex and deeply interesting problem. The more his facts and reasonings are scrutinised, the more widely and entirely will they, in our judgment, compel assent, and the greater will be the consequent salvage of human life."

The article, not being signed, this decided expression of opinion would not be known to have the weighty authority of the President of the Royal College of Physicians; the editor of the Review, therefore, substituted for it the following sentence: "On this point, as on others, Dr. Johnson's facts and reasonings are well deserving of careful and impartial consideration."

In 1871 Sir Thomas Watson brought out a new edition of his Lectures on "The Principles and Practice of Physic," and in the lecture on "Epidemic Cholera" he set forth at length the facts and reasonings which had induced him to change his opinion with regard to the pathology and treatment of the disease. This change of opinion had before been published, and there appeared in the *Lancet*,

Aug. 19, 1871, an article treating on "The Therapeutics of Cholera," from which the following is an extract : "The irrepressible castor oil controversy is threatening to assert itself again. To say nothing of the physician who is chiefly associated with the eliminative theory of treatment, a distinguished medical Baronet, not given to commit errors of judgment or of taste, does not hesitate, every now and again, by somewhat unusual courses, to indicate his adoption of the castor oil plan of treatment, or allow it to be indicated. . . . If this treatment is to have further trial on the strength of the authority of Sir Thomas Watson, we can only hope that Sir Thomas's authority will continue to be entirely dissociated from any personal experience of the treatment, as it is now admittedly unsupported by any personal observation of cases. Sir Thomas Watson's mere authority will undoubtedly cause this practice to be tried more extensively than otherwise it would be, and there is great responsibility in such a use of authority. But the days are gone for enjoining a practice on the strength of mere authority, apart from personal or clinical experience ; and we cannot doubt that the profession will agree with us in thinking that Sir Thomas Watson would have done better to withhold views on this momentous subject till he was able to support them by some facts either within his own observation or which it was in his own power to certify."

I cannot doubt that the unprejudiced members of

the profession will be of opinion that the error of judgment or of taste has been shown, not by Sir Thomas Watson, but by his critic.

When that article was published there must have been on the staff of the *Lancet* a man too old to learn or too proud to acknowledge that his persistent advocacy of the repressive treatment of choleraic symptoms was the result of accepting a theory of the disease which had been proved to be erroneous, and the holding of which blinds men to the mischievous results of the practice which it suggests.

The writer of the article in question speaks of the great responsibility in such a use of authority. Is there no responsibility in the attempt of an anonymous writer in an influential medical journal to discredit such an authority as Sir Thomas Watson?

We know how eloquently in his introductory lecture the great physician expressed his deep sense of the grave responsibility which attaches to a public teacher. He says: "Doctrines and maxims flow abroad from a public teacher as from a fountain, and his faulty lessons may become the indirect source of unspeakable mischief and suffering to hundreds who have never even heard his name. These thoughts fill my mind with an almost painful sense of the obligation imposed upon me by my present office of closely sifting the facts, and meditating carefully the precepts which I offer for your instruction and guidance."

This is the wise and cautious teacher whom the

anonymous leading article writer would subject to a repressive process similar to that which he blindly advocated for the treatment of cholera. One result of such vituperative writing as I have referred to, is to deter cautious and timid men from publishing opinions which they believe to be true, but which they know to be unpopular.

After the publication of my "Notes on Cholera," I received many letters from eminent members of the profession expressing entire agreement with my pathology and practice; but when I suggested that they should give public expression to their approval the reply generally was that they had nothing new to say; my facts and arguments were unanswerable, and required no further confirmation. The fact is that, as the most revered head of the profession had not escaped unseemly censure for having the courage to admit that he had abandoned opinions and a practice which he had discovered to be erroneous and mischievous, they were unwilling to expose themselves to the like unpleasant ordeal. I here reproduce one of the letters above referred to. The writer is physician to one of the largest London hospitals, a Fellow of the Royal Society, and has a great reputation for his original researches in a special department of pathology.

"I have read your book. It is the most interesting medical book I have yet read. Praise from me to you is out of place. But I must say that whatever your theory of the particular disease,

cholera, may lead to, your study of cholera as a piece of pathology is a specimen of work of the highest kind. As a student of medicine I am delighted to see such good work."

This expression of opinion by a very eminent pathologist and a most experienced physician, was very gratifying, and if he had sent a somewhat similar communication to one of the medical journals it would have made the most reckless reviewers cautious in the exercise of their responsible critical functions.

VII. Medical Reviews of "*Notes on Cholera.*"

I now propose to refer briefly to the manner in which the principal medical journals dealt with my "*Notes on Cholera.*"

The *Lancet* did me the honour to devote two leading articles to the analysis and review of the book.* The writer says: "We need not say that the book is well written; and although there is apparent now and again a too great readiness to accept whatever makes for favourite opinions, and to treat lightly an opponent, yet, admitting these faults, Dr. Johnson writes like a serious and a scientific man, and as such we shall try to treat him." The writer, although manifestly hampered by his own views as to the nature and proper treat-

* The *Lancet*, April 18th and June 2nd, 1866.

ment of cholera, gives, on the whole, a fair analysis of the book, but with some remarkable inaccuracies. For instance, in the first article he says: "It is important that we should represent how strongly Dr. Johnson urges that the only bearing of the evacuations on the symptoms is a beneficial one;" yet in the second article it is stated that "Dr. Johnson admits that occasionally the very abundance of the discharges may be a cause of death by exhaustion." This latter statement of my opinion is true enough, but it is quite inconsistent with the previous untrue statement, unless the writer supposed me to look upon death by exhaustion as "beneficial." The reviewer demurs to my theory of collapse; but it is obvious that he has not sufficient knowledge of the forces concerned in the circulation of the blood as taught by all modern physiologists, to see clearly that the action of the pulmonary vaso-constrictors is the only possible explanation of the condition of the heart and lungs which is found *soon after death in the collapse stage of cholera*. With regard to the treatment of the disease the reviewer says: "Our great wisdom in the early treatment of cholera is to keep intact the composition of the blood by restraining the drain from it which almost invariably precedes, and seems largely to cause, the symptoms of collapse."

The attempt to "keep intact the composition of the blood" by preventing the expulsion of the

virulent poison which causes the drain of liquid has not the appearance of “great wisdom,” and its great folly has been proved by ample experience, as I shall presently show.

This controversy with the *Lancet* is now a matter of ancient history. It is probable that the men who from the year 1854 were so persistently opposed to me with regard to the pathology and treatment of cholera, if they are still on the staff of that journal, have changed their opinions.

The writer of a very able review of my “Medical Lectures and Essays,” which appeared in the *Lancet* (March 24 and 31, 1888), could not have been one of my former opponents, for after a complete analysis of the chapter on cholera, he says: “This chapter alone should win for him an honoured place in the history of medicine.”

The *British Medical Journal* (January 27, 1866), in a leading article which occupies nearly three pages of the journal, gave a very able and lucid epitome of my “Notes on Cholera.” In the course of the article the writer, referring to the theory of collapse, says: “We may truly say this of it, that there is perhaps no instance on record in which the theory of a disease founded on physiological and pathological facts more strikingly explains the symptoms and dictates the course of treatment. We therefore think that the profession is greatly indebted to Dr. Johnson for thus enforcing upon their attention at this moment so vital a question.

He offers us a pathological theory which suggests a truly scientific treatment of the disease."

The writer concludes his very careful and complete analysis of the book with the following expression of opinion: "Such, we believe, is a correct exposition of what we must venture to consider the most valuable and original work which this publishing season has added to medical literature."

The *Medical Times and Gazette* (May 5, 1866) gives a fairly complete and accurate analysis of my "Notes on Cholera." The reviewer remarks at the beginning of his notice, that it is "a short but exceedingly elaborate and closely reasoned work," written in vindication of my practice in 1854. From time to time he interposes comments which show that if he is a convert to my pathology he is unwilling to abandon his former creed. He truly remarks upon one point which many of my opponents fail to see. "His aim (the author's) is not to increase the drain from the blood, but to empty the intestinal canal of what is really outside the system proper. . . . His plan is not eliminative in the strict sense then, but only in the subordinate way of removing materials from the bowels which have already been removed from the system." But surely the bowels are a part of the "system," and to facilitate and quicken the expulsion of their poisonous contents is as much a process of elimination as is excretion from the blood. I have seen several cases in which the muscular coat of the bowels has

been so paralysed by opium and the accumulation of their contents that even croton oil has failed to excite evacuations. Then, referring to my theory, the reviewer says: "Let us remark again that good as this may seem—and we have rarely seen so brilliant a piece of pathology—we must judge the practice founded on it separately."

That is what I did with regard to the theory to which my reviewer is inclined. It was not until I saw the deadly result of arresting diarrhœa by opium that I looked about for a theory more in accordance with facts than that which suggested so fatal a practice. Dr. Aitken's well-known text-book is quoted to show that "the facts which Dr. Johnson adduces from the morbid anatomy of cholera, in explanation of the pathology of the disease, are generally admitted." The reviewer admits the "ingenuity" and reasonableness of the theory that the blood is arrested by contraction of the pulmonary arterioles. The general conclusion is that it may be best "to do nothing in the stage of collapse, except nurse the patient carefully and soothe him as much as possible. . . . Little as we can do in blue cholera, the experience of mankind has shown that a great deal can be done in diarrhœa."

Yes, undoubtedly a great deal may be done for diarrhœa, but the vital question is whether that which is done is beneficial or the reverse. In a series of leading articles published later than the

review, the editor claims for the astringent and repressive treatment of diarrhoea the greatest amount of success.

The *Medical Press and Circular* (April 11, 1866) gave a brief but commendatory review of my book. The author says: “Dr. Johnson’s chief object is to disprove the theory that the collapse of cholera is due to the loss of fluids and consequent thickening of the blood, and to point out the faults of practice resulting from this (as he believes) erroneous theory. We think, however, Dr. Johnson considers the views which he combats, and the lines of treatment he opposes, are much more generally received than they really are at the present day. Dr. Johnson deduces very fair and convincing arguments from facts noted by various writers on cholera in support of his views.” The reviewer then describes briefly but clearly my theory and treatment of the disease, and says: “In conclusion we have to recommend Dr. Johnson’s carefully written and neatly got-up little volume, so full of interesting and useful information to all members of the profession.”

It would appear, however, that the editor or some one connected with the journal was of opinion that the reviewer had been too much in favour of my views, for soon afterwards, on May 23, there appeared a leading article, in which the writer says: “Without entering at all into the merits of Dr. Johnson’s pathological theory, which is ingenious and may be true, we at once demur to his practical

corollary. In 1855 the Treatment Committee of the Medical Council addressed a report on the results of the different modes of treatment pursued in epidemic cholera to the President of the General Board of Health."

The general percentage of deaths from each plan of treatment was stated to be as follows :

Of Eliminants	71·7	per cent.
Stimulants	54	„
Alteratives, calomel and opium	36·2	„
Astringents, chalk and opium	20·3	„

The writer of the article thinks these figures conclusive, and he says : " From a fair experience we can confidently recommend full doses of opium, as an almost unfailing specific against the premonitory diarrhœa, while we know of no cure for true cholera save in careful nursing."

Yet the Treatment Committee reported that 79·7 per cent of cholera cases were cured by " astringents, chalk and opium."

I have not before made any public comment on this notorious report, which was published in all the papers, both general and medical, and must have had great influence in deterring fair-minded men from giving a trial to the evacuant treatment.

The obvious, if not the avowed, object of the Committee was to put an extinguisher upon me, my theory and my practice. It may be safely assumed that the Committee and those who professed to try

the eliminative treatment, were as convinced that it was wrong in theory and injurious in practice as was the chairman of the Committee, Sir George Burrows, who, when Sir Thomas Watson publicly announced that he had become a convert to my views, wrote to rebuke him for having "abandoned so many of his early principles." Sir Thomas Watson's reply to his censor was, as he told me: "Although I am advanced in years, I hope I am not too old to learn."

Now, men conscientiously believing that the eliminative treatment was injurious, could not give it a fair trial; for they would not dare to apply it to any but the most hopeless cases. It would not be difficult to go into a cholera ward, and select cases in which, under my treatment, the mortality would be not less than 100 per cent. No particulars of the cases were published, but it must have been in cases of extreme and desperate collapse that the mortality was as high as 71·7 per cent., and on the other hand the statement that with a treatment by "astringents, chalk and opium," choleraic collapse—which all the cases were represented to be—was fatal to the extent of only 20·3 per cent., is sufficient to discredit the entire report. I shall presently refer to some much more trustworthy statistics than these. But before doing so I am anxious to show that the high mortality of cholera during the stage of collapse affords significant evidence that the natural cure of the disease is by a process of elimina-

tion. No drug, not even opium, has so direct and powerful an influence in arresting or checking elimination as the greatly impeded circulation through the lungs, of which extreme contraction of the pulmonary arterioles is the cause, and the condition known as collapse the result. The supply of blood to the left side of the heart and the systemic arteries is greatly reduced, and, as an obvious consequence, the process of elimination by the gastrointestinal mucous membrane is in a corresponding degree lessened. The discharges are always greatly diminished during collapse, and too often their complete suppression is inevitably fatal. It is surprising that any one who has seen much of cholera should doubt that a continuance of the discharges during collapse is an essential condition of recovery from that perilous state.

VIII. Contrasted Results of Evacuant and Astringent Treatment of Cholera.

The most valuable record of the results of various modes of treatment that I know of is contained in a very able and most interesting paper by Drs. M'Cloy and Robertson, published in the 50th volume of the "Transactions of the Royal Medical and Chirurgical Society." The authors state that 375 cases of cholera with collapse were admitted into the Liverpool parish infirmary during the

epidemic of 1866. Of these cases 91 were treated with astringents and stimulants, camphor and iced water, applications of ice and hypodermic injections, with a mortality of 71·42 per cent. 87 cases were treated with castor oil, and a liberal use of food and alcohol, with a mortality of 42·37 per cent. The authors express their belief that food and alcohol given during collapse were injurious, and to some extent counteracted the good effects of the evacuant treatment. 197 cases were treated with castor oil, receiving little or no food of any kind or alcoholic stimulants until reaction was fairly established; of these only 30·45 per cent. died.

It is stated that the cases which were treated by astringents and stimulants occurred for the most part at the very outbreak of the epidemic, and amongst an unfavourable class of patients, but at another institution, under the same mode of treatment, the mortality continued almost as high during the whole period of the epidemic. Again they say “the *sudden* decrease in the death rate which followed the change of treatment pointed to the latter as the cause of the former.”

Some interesting tables and details of typical cases are given. For instance, a boy aged ten recovered from collapse of such gravity and with so extreme an arrest of the circulation that he had gangrene of both hands, which necessitated amputation, followed by complete recovery. With regard to diarrhœa, the authors say, “Our experience was

very extensive. Several thousand cases came under our observation in the different dispensaries connected with the West Derby Union, and in the Liverpool parish infirmary. Among these were many who doubtless would have recovered under any mode of treatment, or by the *vis medicatrix naturæ* alone. There were many, too, of a most severe choleraic type. The treatment adopted was generally evacuant in its nature, and consisted in the administration of castor oil, calomel, rhubarb, or magnesia. In every case relief was effected quickly, pleasantly, and safely. It was but seldom that more than two or three doses of castor oil were required. In one of the public dispensaries (Bootle) many cases of diarrhœa were treated with evacuants, and the testimony of the medical officers is in accordance with our own. ‘We certainly had less trouble with the evacuant mode of treatment.’ We never saw a diarrhœa patient treated from the commencement of his attack, require subsequent removal to the hospital. In a large proportion of our cases (of cholera) there was premonitory diarrhœa, which had been treated, often for four or five days, with astringents. Diarrhœa patients undoubtedly recover when treated with astringents, but the recovery is not consequent on the arrest of the discharges, as these are invariably restored before the patient feels well.” The authors state emphatically that “whatever the treatment adopted the result was the same—recovery never occurred

without the continuance of the intestinal discharges or their restoration, if previously arrested."

I have no doubt that this important paper—although, so far as I know, Sir Thomas Watson is the only subsequent writer, except myself, who has quoted it and recognised its value—has silently exercised great influence upon the mind of the profession.

In the *Lancet* of August 18, 1866, there is a report by Dr. Ferris of 201 cases of diarrhœa successfully treated at the Bloomsbury dispensary, mostly by evacuants, which Dr. Pidduck, the physician to the charity, had found successful in previous epidemics. A pill of half a grain of calomel was followed in two hours by half an ounce of castor oil. If next day the purging continued, a slightly astringent mixture was given, "but this was necessary in only a small number of cases." All the patients recovered, and the report states that a considerable number of them had been previously treated at other institutions unsuccessfully by astringents. "One man had been ill for a week, taking astringents the whole time; after the above treatment he went to work the next day quite well." Twenty-two of the cases were treated by coloured mint water, of whom only two returned for further treatment.

I challenge the advocates of the repressive treatment of choleraic diarrhœa by opium and other astringents to produce evidence in support of their

theory and practice as conclusive as that which has here been adduced in favour of evacuants. What is often said is that by the use of astringents some cases of diarrhœa appear to have been prevented from passing into collapse, but there is no recognition of the fact that collapse has often followed directly upon the arrest of the discharges by opium.

I have, during the last forty years, treated hundreds of cases of diarrhœa by evacuants, and *not one so treated has passed into collapse*. Contrast the result of the several thousand cases in Liverpool and the 201 cases at the Bloomsbury dispensary with the disastrous results of the opposite treatment by two eminent French physicians, Drs. Briguet and Mignot.* They treated in the hospital 200 cases of diarrhœa, at the very commencement of the attack, by from 15 to 30 drops of laudanum, the dose to be repeated in an hour, and followed by opiate enemata if necessary. The result was that 26—that is 13 per cent.—passed into collapse. It is probable that *most* of them would have passed into collapse were it not for the fact that the outward flux from the blood impedes the absorption of the drug, and the natural curative efforts eject the morbid poison, together with the narcotic by which the attempt is made to arrest elimination.

Dr. Koch's experiments on guinea-pigs afford an instructive illustration of the influence of opium in

* "Traité pratique et analytique du Cholera Morbus, 1850."

preventing the escape of the cholera poison.* After introducing the cholera bacillus into the stomach of the animal, he injected into the peritoneal cavity a narcotic dose of tincture of opium—his object being to render “it possible for the comma bacillus to remain longer, and gain a footing in the intestine.” In this manner he produced fatal cholera in 30 out of 35 guinea-pigs experimented on.

Koch's experiments have been repeated with similar results by Dr. Neil Macleod.† He induced cholera in 38 out of 54 guinea-pigs by injecting cholera bacilli into the stomach and opium into the peritoneum, the object being to check peristalsis and to allow time for the multiplication of the organisms. It must be obvious to any unprejudiced observer that the patients of those who endeavour to arrest the choleraic discharges by opium are subjected to the same kind of treatment as the before-mentioned guinea-pigs. There is, however, this important difference, all the opium injected into the peritoneum, or beneath the skin, is absorbed, but much of that taken by the mouth for the arrest of diarrhœa is ejected without entering the circulation; the latter method, therefore, is less deadly in its results than the former.

The following case affords an instructive illustration of the effect of two opposite modes of treatment on the same patient. In August 1866, I saw, with

* *British Medical Journal*, January 1886, pp. 63-4.

† *Lancet*, March 9, 1889.

Dr. Halse and his partner, a youth aged 15, who had been seized with diarrhœa after drinking the water from a pump in the Temple, water which was believed to have been the cause of other cases of cholera.

The diarrhœa had been stopped by two doses of opium. Then after a few hours' cessation of the diarrhœa he was again seized with purging, which, being stopped by opium, he rapidly passed into collapse.

When I first saw him at 11 A.M. on August 6, his eyes were sunk, his skin and tongue were icy cold, his pulse was too feeble to be counted, and cramps were very severe. There had been no purging for several hours, but on palpation and percussion the intestines were found to contain a considerable amount of fluid. There was a state of general torpor, and no natural effort to evacuate the bowels. He was ordered to take half an ounce of castor oil every two hours.

At 8 P.M. he was reported to have vomited once and to have passed three foetid rice-water stools. The intestines were less distended, and he was free from cramps, but in other respects the condition was unchanged. He was to have one or two doses of oil during the night.

On the seventh, at 9.30 A.M., he had rallied completely, his countenance being natural and his skin warm. There had been five or six bile-tinged stools during the night, and the intestines were now every-

where resonant on percussion. After this he was drowsy for a few hours, and on the eighth a rash of roseola appeared on the trunk and limbs, but he made steady progress towards convalescence.

This case is more instructive than any number of mere statistics. The opium acted upon the boy as it did upon Koch's guinea-pigs. The diarrhœa having been arrested a second time, collapse directly followed, and would have been fatal but for the evacuant action of the castor oil. I have never seen a case in which the life-saving action of a purgative was more manifest. If the patient had been simply "nursed and soothed," which some writers declare to be the only treatment for collapse, he would have gone to his grave with distended bowels, as many a collapsed patient has gone.

Dr. Halse had a senior partner who was much too old to learn. He could not be made to see that his opiate treatment had placed the patient's life in danger; and he looked on in a pitiful state of fear and trepidation, from a dread that castor oil would cause fatal exhaustion. This old gentleman was a type of a class of practitioners at that time very numerous, but now, it is to be hoped, nearly, if not quite extinct.

IX. Dr. Sutton's Report on the Post-mortem Appearances in the Stage of Collapse.

An important paper on the condition of the heart and lungs after death, in the collapse stage of cholera, was contributed to the fourth volume of the "London Hospital Reports," by the late Dr. Sutton. Two physicians of Guy's Hospital, who have since died, questioned the accuracy of my report of the post-mortem appearances. They both denied that there was a great accumulation of blood in the right cavities of the heart and the pulmonary artery, and that the minute tissue of the lungs was remarkably anæmic after death in collapse.

One of them, who was notorious for treating grave subjects with levity, said of me (*Lancet*, January 19, 1867) "We believe him to be as honest as he is mistaken, and we can scarcely pay a higher compliment to his uprightness."

As I was not content to be considered honestly mistaken, it was satisfactory to find that Dr. Sutton's report of the post-mortem appearance in fifty cases of death in collapse, during the epidemic of 1866 was in exact agreement with the description which I had published, and the accuracy of which was denied by my opponents. Dr. Sutton says of the heart: "The right side was seen to be very much distended. This was particularly noticeable in the greatly distended condition of the right auricle and the auricular appendage. . . . The left ventricle

was usually contracted, often firmly so; and on cutting off the apex of the ventricle and squeezing it, the cavity was seen to be empty, or contained very little blood indeed."

"The lungs weighed very much less than normal. In some cases they were very pale in the anterior two-thirds, and of a darker colour in the posterior third. The pale portions on exposure to the air rapidly became of a scarlet colour. In other cases the lungs throughout were of a dark red colour, but both the pale and the dark-looking lungs were dry, and on pressure gave but very little blood. Dark thick blood was seen flowing out of the branches of the pulmonary artery."

This condition of the heart and lungs is the result of the stream of blood during the collapse stage of cholera having been arrested before it had reached the pulmonary capillaries; and extreme contraction of the arterioles is the only possible explanation of the arrest.

It is important to bear in mind that if the large veins of the neck are wounded in opening the chest, the distended right ventricle rapidly empties itself. Dr. Sutton mentions one case (*loc. cit.* p. 493) in which the right ventricle was thus emptied in two or three minutes. This result of wounding a vein gives the explanation of the marvellous relief afforded by venesection during choleraic collapse, as reported by several of the most trustworthy practitioners in India (see p. 10).

If the inspection of the body is delayed for many hours the distension of the right cavities is found to be less than when the examination is made soon after death. The obvious explanation is that the vital contraction of the pulmonary arterioles which arrests the circulation, ceases after death, and thus allows the elastic resiliency of the distended pulmonary artery and the right side of the heart to drive the blood onwards into the pulmonary capillaries and veins, and to the left side of the heart. The dark colour of the lungs which are light in weight and contain but little blood, is explained by the backward engorgement of the *bronchial* veins and capillaries, in common with the whole systemic venous system.

I gladly acknowledge that by this report Dr. Sutton, with whom, respecting another subject I had a sharp controversy, has made a valuable contribution to the morbid anatomy of cholera, which quite proves the accuracy of Dr. Parkes' original description of the appearances found after death in collapse, and has thus silenced further controversy on this subject.

X. Mr. Macnamara's Theories and Practice.

One of the most conspicuous and strenuous advocates of the astringent treatment of choleraic diarrhoea has been my friend Mr. N. C. Macnamara. He has written three books on cholera, and he

is the author of the article "Cholera" in the two editions of Sir Richard Quain's "Dictionary of Medicine."

It is interesting to compare and contrast the treatment recommended in his article with that advised by Dr. George Oliver in the article "Diarrhœa" in the same dictionary. Mr. Macnamara says: "In the first stage of Asiatic cholera we should endeavour to stop the purging, and without doubt opium is the drug upon which we may with the greatest confidence rely for effecting this purpose." He goes on to say that he was in the habit of giving at once a pill containing a grain of opium and four grains of acetate of lead, and if the diarrhœa continued, a second and even a third pill was given.

Dr. Oliver, on the other hand, says: "In choleraic diarrhœa the best results are obtained from castor oil guarded by a small dose of laudanum at the commencement, and repeated if the disease is severe, while astringents and opiates alone are withheld until the bowels are relieved of offensive materials."

Mr. Macnamara, with his fear of exciting vomiting, directs that "the patient should be prohibited from swallowing water or any other liquid than that which he gets from the ice," which he is allowed to take *ad libitum*. Dr. Oliver, on the contrary, says, "Vomiting should be encouraged by copious draughts of warm water, and if need be by emetics of mustard or ipecacuanha."

The two methods of treatment are as wide as the poles asunder. One endeavours to suppress the natural efforts to expel a poison, the other has for its object to aid those efforts; and the editor must surely have experienced some difficulty in allowing such irreconcilable articles to appear in his dictionary. He, doubtless, saw that they were antagonistic, and apparently left it to his readers to choose between them. Surely common-sense, apart from any profound theory, would pronounce in favour of Dr. Oliver's treatment.

Mr. Macnamara's voluminous writings, in the course of which he makes repeated reference to his large experience of cholera in India, have probably had considerable influence upon many of his readers. I propose, therefore, to set forth and to criticise some of the extraordinary pathological theories for which he has made himself responsible.

His first book, "A Treatise on Asiatic Cholera," was published in 1870, and is dedicated to the Duke of Argyle, at that time Secretary of State for India.

In this work his theory of cholera is in substance as follows. A poison, or as he says, *a specific organic matter* from a previous sufferer being swallowed, does not enter the blood, but by a local action destroys the epithelium of the intestinal canal. "The epithelium being thus destroyed the serum of the blood is allowed to drain away from the capillary arteries (*sic*) of the intestines, and at the same time venous absorption is prevented from taking place

through the intestines. Hence the symptoms of cholera. After a time, in favourable cases, the blood, by deprivation of its water having been greatly thickened, coagulates in the intestinal capillaries. This stops the further drain of liquid. Thus the epithelium is reproduced, and so absorption, which had been rendered impossible during the absence of the epithelium, recommences, the water is restored to the blood, and the patient recovers."

This strange, mechanical theory was based upon the observation of an eminent microscopist, that after death from cholera the intestinal villi were found denuded of epithelium.

This denudation was soon shown to be the result of maceration of the mucous membrane in the morbid contents of the bowels after death, so that the removal of the epithelium is a result, and not the cause of the escape of fluid into the intestines. But even if the denuding process had occurred during life, it is inconceivable that the copious out-pouring of liquid from the blood could be so caused. The author has since admitted that the epithelium of the villi *is not destroyed during life*.

If the remarkable theory that the poison of cholera has a purely local action on the intestines without entering the blood, had a foundation in fact the practical inference would be that the noxious stuff should be expelled by a purgative, and not retained there by an opiate until it has destroyed the intestinal epithelium.

It is a peculiarity of Mr. Macnamara's method that every fact which appears to be inconsistent with his "dehydration," theory of collapse is denied or is questioned. Thus he says, that "if the body is examined soon after death from collapse, the left side of the heart contains as much blood as the right." He maintains that the distension of the right side of the heart depends on "the water of the tissues draining into the venous capillaries and right side of the heart after death in collapse, the whole venous system being full of blood from this cause." This is a good illustration of Mr. Macnamara's unphysiological method. Although he believes that during life the tissues are "well-nigh dehydrated" he imagines that after death they pour into the veins such an abundance of water as to distend the entire venous system.

The fact is that the sooner after death the inspection is made, the greater is the distension, not only of the right side of the heart and the whole systemic venous system, but of the pulmonary artery to its minutest subdivisions. The explanation of this I have before given. (See p. 39.)

In Quain's "Dictionary of Medicine," Mr. Macnamara gives his version of the condition of the heart's cavities, but without the astonishing explanation which I have just now quoted, and to this account of the appearances, the editor, in the second edition, appends the following foot-note :

"On this subject see the chapter on epidemic

cholera in Dr. George Johnson's *Medical Lectures and Essays*."

It would appear, therefore, that the editor doubted the accuracy of his contributor's discription of the post-mortem appearance in cholera. It is probable, too, that this editorial note made an impression on Mr. Macnamara, for in his history of *Asiatic Cholera*, which was published in 1892, probably after the article on "Cholera" was in print, the author says quite correctly: "The heart, as a rule, is found distended with dark blood on the right side, its left side being empty. The jugular veins, and the vena cava, together with the coronary veins, are full of blood, but the aorta and other arteries are empty, with the exception of the pulmonary artery." Then as to the lungs, he says they are "below their normal weight, and in the greater number of cases are collapsed, and found lying back against the spine. On section they appear dry, containing but little blood, and that is confined to the pulmonary arteries, and their branches, the capillaries and veins of the lungs are empty." It would have been more satisfactory if this correct description of the heart and lungs immediately after death in the collapse stage of cholera had been given in the article "Cholera" in Quain's "Dictionary of Medicine," the second edition of which was published in 1895. It is obvious that the appearances described are inconsistent with the theory that the collapse of cholera is the result of dehydration of the blood and tissues.

The sudden onset of collapse in the most malignant choleraic attacks is inexplicable by the most rapid dehydration process; therefore, Mr. Macnamara doubts the occurrence of such cases. In my "Notes on Cholera," I quote from a report on cholera in the Black Sea fleet in 1854, the statement that amongst robust English sailors "the attacks in many instances were so sudden that men fell as if they had drunk the concentrated poison of the upas tree." This he says is "high-flown language." But I find in Mr. Macnamara's treatise on "Asiatic Cholera" language even more sensational.

Thus at p. 17, in an extract from the "Proceedings of the Bengal Medical Board" for 1817 the disease is spoken of as "running its course generally in a few hours, and sometimes in a few minutes."

At p. 19 Dr. Corbyn reports that in 1818 "some of the Governor-General's servants dropped down dead behind his chair." And at p. 24 there is an extract from Jameson's report to the effect that at Bunderpoor in 1818 "the patients were described as having been knocked down dead as if struck by lightning." Surely lightning beats the upas poison in speed. Such cases must have been of exceptional malignancy; but the sudden onset is explicable by arterial contraction, as every one can understand who has studied the effect of breathing an azotic gas, such as nitrous oxide or nitrogen.

The result is, that in less than two minutes the circulation is completely arrested by the same nervo-vascular mechanism as that which stops the flow of blood in cholera. Again, in the Appendix to Mr. Macnamara's book, which he refers to as "a valuable record," "Dr. Bruce says: "It is still a favourite opinion with many, that the amount of discharge and collapse stand in the direct relation of cause and effect, but experience is opposed to such a conclusion; for who has not seen marked cases in every epidemic where there was neither purging nor vomiting sufficient to cause any degree of collapse, and yet these cases have sunk at least as rapidly as others where the discharges were profuse." He then refers to the case of a man who "stated that he had one copious fluid motion, after which, feeling unwell, he came to the hospital immediately. He vomited for the first time in my presence as he was taken out of the dooly. I found him in a state of collapse, and he was dead in six hours from that time, having been purged again only once: this last was a pure, flocculent congee motion, but it was only his second; and in the post-mortem examination an unusually small amount of congee fluid was found in his intestines." Dr. Bruce also refers to some cases in which, "although the purging and vomiting were checked without difficulty, the collapse steadily increased." Yes, the collapse increased *because* the purging and vomiting were checked.

How long a time must elapse before it is universally known that no patient comes out of collapse without a continuance of the discharges in a greater or less degree?

If there are any facts which are inconsistent with Mr. Macnamara's theory, it is so much the worse for the facts. They are simply ignored. The continued secretion of milk by nursing mothers during collapse has been recorded by numerous observers, from Magendie in 1832 to the medical officers of the London Hospital in 1866. (See p. 8.)

Our author says: "With regard to the supposed secretion of milk in cholera, I can only say from my own experience that the mother's milk ceases as collapse comes on. If, as some state, it increases I cannot understand where its watery element comes from." I am not aware that any one has stated that it *increases*, but only that it is not suppressed, for reasons which I have given; but Mr. Macnamara, believing that little water is left in the body, has persuaded himself that the continued secretion of milk is impossible, and therefore doubts a repeatedly demonstrated fact.

But Mr. Macnamara's statement of the influence which loss of water has upon the buoyancy of the body is the most surprising of his many inaccuracies. In his last published work ("History of Asiatic Cholera," at p. 62) he says: "The blood and tissues of the body are well-nigh dehydrated, and if the patient is placed in a large bath it is not

easy to keep his body under the water; it floats like a cork." Here it is evident that the writer fails to distinguish the actual weight of the body from its specific gravity. Every schoolboy would know that in proportion to the decreased weight of the body by loss of water, its specific gravity must be increased; yet Mr. Macnamara writes as if he had actually seen the body of a choleraic patient *floating like a cork*. Since he misunderstands so simple a physical principle as this, we can appreciate his incapacity to interpret the more complex physiological phenomena of cholera.

In this same book there are indications that the author is, after all, losing faith in his own theory of collapse. He says (p. 59): "With reference to the poison which we believe the cholera bacillus produces, several theories have been advanced to explain its action. Among these theories that advocated by Sir George Johnson takes a prominent position, and by many pathologists is held to be best capable of accounting for the symptoms and the *post-mortem* changes found after death from cholera" (*sic*).

He then gives a brief but sufficiently accurate account of my theory, but follows it up by some futile objections, which are no more worthy of notice than his statement that the body of a cholera patient "floats like a cork." I have felt it to be a duty to deal thus seriously and at length with Mr. Macnamara's fanciful opinions; for as Sir

Thomas Watson says : " The subjects with which we have to deal are not matters of mere speculative curiosity or intellectual amusement—to be taken up to-day and dismissed perhaps with unconcern to-morrow ; but they involve questions of life and death." No man with more than an elementary knowledge of physiology would have put forth such wild theories as those to which I have referred ; but Mr. Macnamara's conviction, that the opiate treatment of cholera is the only successful method is so strong that he is blind to all facts and arguments which are opposed to that opinion. He quotes Dr. Koch's experiments on guinea-pigs, the object of the operator being, by means of an opiate injection, " to retain the cholera bacillus as long as possible within the intestinal canal." But he fails to see that by his opiate treatment he is repeating on the human subject an experiment, in principle, identical with that of Koch. It might be well if in future the repressive treatment of choleraic diarrhœa by opium were known by the distinctive name of *Koch's guinea-pig treatment*.

Mr. Macnamara in his " History of Asiatic Cholera" (p. 63), under the head of " Treatment " says : " Having gone through a severe attack of cholera I can testify to the unspeakable relief which ice affords to a person passing through an attack of this terrible disease. In my own case the onset of the disease was so sudden, and the vomiting so incessant, that everything I took was immediately rejected, and

consequently it was useless taking medicine ; but I chewed and swallowed pounds of ice to my infinite comfort."

The author makes no mention of having had more than one attack of cholera, yet it is certain that the preceding account could not refer to the attack which he had when he was assisting me in the cholera wards in 1854, of which he gave me the following notes for publication a few days after his recovery :

"The following is the report of a severe attack of diarrhœa I was seized with on September 4. I was in my usual state of health up to the evening of the 4th inst. and having taken a particular interest in the cholera patients in our hospital, I had for three or four days previous to the evening on which I was myself taken ill, almost lived in the cholera wards.

"On Monday night at about eleven o'clock I awoke with violent pain and spasm in my stomach, and a most distressing sense of oppression, with an inclination to vomit. These symptoms were quickly followed by cramps in my hands and feet. I at once got up and took an emetic, and as soon as the vomiting which it excited had ceased I followed it up with an ounce dose of castor oil. In about half an hour's time I was violently purged, and so much relief did I experience from it that I repeated the dose of oil during the following hour. From this time until eight o'clock in the morning I was purged

and vomited so freely that I became quite exhausted ; yet I suffered little pain, and had lost that dreadful sense of oppression to which I before alluded. I then had two hours' sleep, and awoke much refreshed.

Throughout the day my bowels were opened several times, and I vomited once or twice after taking food ; but in the evening I went down to Blackheath, passed a most comfortable night, and the next day returned to town ready to resume my duties, although much pulled down by my severe though short illness.

“C. N. M., M.R.C.S.

“KING'S COLLEGE HOSPITAL,

“*Sept.* 1854.”

“The dreadful sense of oppression” was probably due to the commencing embarrassment of the pulmonary circulation by the cholera poison. From this he was speedily relieved by the prompt recourse to evacuant treatment. If instead of the emetic and castor oil, of which, however, he took twice as much as there was need for, he had taken opium, the result would have been very different.

It is remarkable that Mr. Macnamara should for a number of years have been so strenuous, and, as I think I have shown, so unreasonable an opponent of a principle of treatment which was the means of saving his own life.

XI. The contrasted History of opposite Methods of Treatment.

The contrast between the history of the repressive and the evacuant treatment of cholera may be briefly stated.

The theory that loss of water is the chief cause of collapse was the result of an imperfect observation of facts. This theory suggested the employment of opium, and has blinded those who hold it to the evil results of the treatment. On the contrary, the evacuant treatment was successfully resorted to after the disastrous effects of opium had been witnessed.

Then, subsequently, a careful investigation of all the facts of the disease led to the conclusion that the main cause of collapse is a greatly impeded flow of blood through the lungs. When once it is clearly seen that this impeded circulation is a result of contraction of the pulmonary arterioles excited by a poison in the blood it is manifest that the attempt to prevent the escape of the poison by opium is irrational, and must be injurious. The different results of the two opposite modes of treatment—the astringent and the evacuant—are completely explained by the true theory of collapse.

I have never supposed or stated that evacuants of any kind are essential for the cure of choleraic diarrhoea. The *vis medicatrix naturæ* suffices for the

recovery of most cases, if only the patients do not continue to drink the poisonous water which in many cases had excited the diarrhœa. As many patients recover in spite of astringents, so many more would recover if they were treated by coloured water.

It has sometimes been asserted that an attack of cholera has been caused by a rhubarb pill or a dose of castor oil. I can give a parallel to this *post hoc propter hoc* argument. Many years ago, when I was seeing out-patients at the hospital, a woman brought a feverish child, for whom I prescribed a saline mixture. Two days afterwards the mother returned, and said in a complaining tone, "Your medicine has brought out the small-pox." Truly the small-pox had come out, but assuredly it was in the blood before it appeared in the skin. So when symptoms of cholera follow the action of an aperient the morbid poison was before in the blood, and caused the derangement of health for which the dose was taken. It is about as probable that small-pox could result from a croton-oil liniment, as that a specific disease like cholera could be caused by a purgative.

XII. The Action of various Remedies.

Some practitioners look upon diluted sulphuric acid as a valuable remedy for diarrhœa, and call it an astringent. If astringent at all, it is so in a different manner and degree from opium, and when

given in full doses it often excites griping and diarrhœa. On the other hand, a chemically opposite remedy—namely, *carbonate of soda*, in mint tea—was reported to be very efficacious by the late Mr. Wakefield, who stated in a letter to the *Times* that during the epidemic of 1854 he had thus treated a large number of cases of diarrhœa amongst the prisoners in Cold Bath Fields, and not one had passed into collapse.

Some practitioners are of opinion that the undoubted efficacy of calomel is due to its direct action on the liver, while others suppose that it has an antiseptic influence; but it would seem that even the largest doses of this insoluble compound that could, with safety, be given would have but an infinitesimal antiseptic influence on the copious intestinal discharges. Calomel *in powder* is an excellent substitute for castor oil when frequent vomiting prevents the retention of the latter. In such cases calomel appears to arrest the vomiting by reversing the action of the stomach. A sagacious friend of mine said to me many years ago something to this effect: "Many men would have more faith in castor oil if you could speak of it as being not merely an evacuant, but as having a specific *castor-oilative influence*." I can understand this craving for a specific curative agent, but I do not share the feeling.

**XIII. Dr. Burney Yeo and Dr. Clemow
on the General Adoption of the
Eliminative Treatment.**

I have the satisfaction of finding that, after forty years of controversy, the evacuant or eliminative treatment is now being generally looked upon not only as rational and scientific, but in practice most successful.

Dr. Burney Yeo in his "Manual of Medical Treatment" (4th ed., vol. ii. p. 696), after referring briefly to the pathological theory, says: "It will be seen from these indications that the rational and scientific treatment of cholera is now admitted nearly on all sides to be, as Sir George Johnson insisted many years ago, mainly *eliminative* and *antiseptic*. 'Purgation and antiseptis are to some extent interchangeable terms.' When cases of cholera are seen in the first stage, when nature is making an effort to throw off the poison by the bowels, we must aid the natural effort by the administration of an unirritating purgative; and if we can at the same time combine with it an intestinal antiseptic, we shall be fulfilling very completely the first indication. Castor oil is one of the best purgatives for this purpose. Calomel is also valuable, and it is an antiseptic as well as a purgative. Gregory's powder, together with a few grains of calomel, is suggested by Sir George Johnson if, owing to vomiting, castor oil cannot be taken."

“Treatment on these lines in the early stage has been found to be the most successful in the recent epidemics in Germany, Russia, and Italy. One Russian physician began the treatment with twenty grains of calomel with an ounce of castor oil, and he reports that the mild cases recovered quickly, and so did many in the algid stage. Another started with a purgative dose of calomel combined with naphthalin, and continued with smaller doses of each. In Hamburg mild threatening cases were stopped by an initial dose of castor oil, and in more advanced stages calomel in $1\frac{1}{2}$ grain doses, or in repeated small doses of $\frac{1}{8}$ to $\frac{1}{6}$ grain, was found to answer well. In severe cases, those of well-marked general intoxication, we are informed, ‘calomel was the only drug that held its own.’

“In the reports of recent epidemics there is a general condemnation of the use of opium to arrest the diarrhoea in the early stage; cases so treated did worse than under any other treatment. The early use of opium increases the risk of the absorption of the cholera poison, and favours its retention within the body.”

This, then, according to Dr. Burney Yeo, is the principle of treatment which is now nearly on all sides admitted to be not only rational and scientific, but successful. I confess that I read this statement with much satisfaction, for I know that Dr. Yeo has had ample opportunities to form a sound judgment as to the general opinion of the profession on this

subject ; and his excellent practical work has had a very large sale, having gone through four editions within two years ; it must now therefore be in the hands of several thousands of practitioners.

Dr. Frank Clemow, an English physician practising in St. Petersburg, has published an interesting work on “The Cholera Epidemic in 1892 in the Russian Empire.” In this work he says (p. 90) : “The diarrhoea being looked upon as a safety-valve whereby the system got rid of large quantities of the poison that would otherwise be absorbed, was within certain limits encouraged. No attempt was, as a rule, made to check it by opium or astringents.” And in a private letter with which I have been favoured by the author he says : “The St. Petersburg physicians were in entire accord with your own teaching as to the necessity for purgatives as opposed to astringents in the treatment of cholera.”

If the general body of Continental practitioners have been more ready than the English to adopt the evacuant treatment of cholera, this may perhaps be due to the fact that the dehydration theory of collapse, which suggests a repressive treatment, has never been so generally accepted abroad as at home, where, for a time, it did an incalculable amount of mischief.

The final result, then, of the cholera controversy is that the dehydration theory of collapse has been replaced by one more in accordance with facts and

with physiology, and the injurious astringent treatment has been superseded by the beneficent principle of elimination. In other words, it has at length come to be generally acknowledged that it is a more rational and a more successful practice to assist than to impede the ejection of a morbid poison.

In 1892 the Local Government Board requested the Royal College of Physicians to give directions for the treatment of diarrhœa and cholera. Detailed instructions were drawn up by the President, Sir Andrew Clark, and accepted, with little modification, by a large meeting of the Fellows. Amongst other directions is the following: "As soon as possible after looseness of the bowels has begun, take in capsules, or in hot milk, or in any other manner preferred, two teaspoonfuls of castor-oil."

This was a step timidly taken in the right direction, which a few years before would have been disapproved, and it was followed by detailed instructions which at the present time—so rapid has been the change of opinion and practice—would be considered unnecessary and even undesirable.

XIV.

DIRECTIONS FOR THE TREATMENT OF DIARRHŒA AND CHOLERA.

The leading principle that should guide the treatment of cholera has been clearly indicated in the preceding pages, but I have thought it desirable to give the following detailed directions for the treatment of the disease in all its stages.

Diarrhœa, during an epidemic season, is in many instances, though not in all, an early stage or a mild form of cholera; and in the great majority of cases of actual cholera, an attack of bilious diarrhœa marks the onset of the disease. A diarrhœa, when it is not the actual beginning of cholera, will weaken the patient, and so may predispose him to suffer from the more serious form of disease. *Diarrhœa, therefore, during an epidemic season, ought not to be neglected even for an hour.* That plan of treatment for diarrhœa is obviously the best which most speedily and completely puts a stop to the disease, without subsequent ill effects.

It may be stated as a general proposition, that

the immediate cause of diarrhœa or looseness of the bowels is the presence of offending materials in the alimentary canal. These offending materials are of various kinds in different classes of cases. In one case, unwholesome and undigested food is the exciting cause of the purging; in another case, a large and unnatural accumulation of the fœculent contents of the bowel; while, in another class of cases, noxious secretions are poured from the blood into the bowel, in consequence of the action of a morbid poison upon some of the ingredients of the blood. To this last class of cases belongs what is called *choleraic diarrhœa*.

The most reasonable theory of choleraic diarrhœa is that a morbid poison enters the blood either with the air through the lungs, or with the food and drink through the alimentary canal; and that this poison excites certain changes in the blood, in consequence of which some blood-materials are spoiled, and thus rendered not only useless, but noxious. These morbidly changed blood-materials are then discharged from the blood-vessels through the mucous membrane of the stomach and bowels, and are ultimately ejected by vomiting and purging.

Various as are the remote and primary causes of diarrhœa, this one condition is common to all classes of cases—viz., that the contents of the bowel are unnatural and offensive. These offending materials are the immediate cause of the purging; and they

must be expelled from the bowel before the diarrhœa can come to an end.*

From the above considerations we deduce one important and guiding rule of treatment, which is this: *not to attempt by opiates, or by other directly repressive means, to arrest a diarrhœa while there is reason to believe that the bowel contains a considerable amount of morbid and offensive material.* It is certain that these offending materials must be cast out from the bowel before the diarrhœa can permanently cease. The effect of an opiate at this stage, if, as frequently happens, its absorption be not prevented by the rapid outward flux of liquid from the blood into the stomach and bowels, is to prolong the disease, and to increase the risk of mischief, from the retention and re-absorption of the morbid contents of the bowel. If the opiate have the effect of retaining *within the blood-vessels* some of the morbidly changed blood-constituents, this astringent action will probably be more injurious and even deadly than the retention of morbid secretions within the bowel. The abrupt arrest of the discharges by opiates is not infrequently followed by fatal collapse.

The purging is the natural way of getting rid of the irritant cause. We may *favour* the recovery by directing the patient to drink copiously any simple diluent liquid—water cold or tepid, toast-

* We need not here take into consideration those cases of diarrhœa which result from ulceration or other local disease of the bowel itself.

water, barley-water, or weak tea; and we may often *accelerate* the recovery by sweeping out the alimentary canal by some safe purgative, and then, if necessary, soothing it by an opiate. Castor oil, notwithstanding its unpleasant taste, is, on the whole, the safest and best purgative for this purpose. It has the advantage of being very mild and unirritating, yet withal very quick in its action. A tablespoonful of the oil may be taken, floating on cold water or in hot milk or any other simple liquid which may be preferred by the patient. A mixture of orange-juice or of lemon-juice with water forms an agreeable vehicle for the oil. Some prefer to take it with weak brandy and water, and there is no serious objection to this mixture, but it is very likely to excite vomiting. Another mode of giving the oil is in the form of an emulsion, with mucilage and cinnamon water, which very completely covers and disguises the taste of the oil. If the dose be vomited, it should be repeated immediately; and the patient should lie still, and take no more liquid for half an hour, by which time the oil will have passed from the stomach into the bowels. Within an hour or two, the oil will usually have acted freely. Then a tablespoonful of brandy may be taken in some thin arrowroot or gruel; and if there be much feeling of irritation, with a sense of sinking, from five to ten drops of laudanum may be given in cold water. These means will suffice for the speedy arrest of most cases of choleraic diarrhœa.

If the patient have an insuperable objection to castor oil, or if the oil cannot be retained on the stomach, ten or fifteen grains of powdered rhubarb, or a tablespoonful of the tincture of rhubarb, or a teaspoonful of Gregory's powder, may be substituted for the oil.

When vomiting is very frequent three grains of calomel in powder, washed down with cold water, may be the best form of purgative.

If the diarrhœa has continued for some hours, the stools having been copious and liquid; if there is no griping pain in the bowels, no feeling or appearance of distension of the intestines—the abdomen being flaccid and empty, and the tongue clean—we may conclude that the morbid agent has already purged itself away. There will, therefore, be no need for the castor oil or other laxative, and we may immediately give the brandy in arrowroot, and the laudanum, as before directed. The rule in all cases is, *not to give the opiate until the morbid poison and its products have for the most part escaped; not to close the door until "the enemy" has been expelled.* While there are some cases in which the evacuant dose is not required even at the commencement of the attack, there are many more in which the opiate is unnecessary in the later stage. In some cases of severe and prolonged diarrhœa it may be necessary to repeat the oil and the laudanum alternately more than once, at intervals of three or four hours. Practical skill and

tact are required to discriminate these cases. It must be borne in mind that when the choleraic secretions are being actively poured out from the blood-vessels, the bowel, though it may have been completely evacuated by a dose of oil, may quickly again become filled with morbid secretions, and hence the need for an occasional repetition of the evacuant dose.

With reference to the use of opiates in the treatment of diarrhœa, the principle to be kept constantly in view is this: *opiates are useful to soothe irritation after the evacuation of the bowel; they are useless and even dangerous when the blood is poisoned or the bowel filled with morbid secretions.* Opiates, in the early stage of diarrhœa would be more frequently and decidedly injurious were it not for the fact that, their absorption being prevented by the active eliminative efforts, they are quickly expelled with the morbid secretions, and they are therefore powerless to arrest the discharges. There is one symptom for which opium has often been given, but which especially forbids its use, and that symptom is *cramp of the muscles*. This cramp shows that the poison which excites it is still in the blood. The painful spasms can be effectually relieved only by the escape of the poison. An opiate dose might hinder the exit of this poison from the blood, and thereby induce that perilous, though painless, spasm of the small arteries of the lungs which is the essential cause of collapse.

It has been a common practice to give calomel and opium combined. The effect of this combination will depend upon the proportions of the ingredients. Equal weights of opium and calomel will have, for the most part, a narcotic and astringent action, but in proportion to the preponderance of the calomel the operation will be evacuant and purgative.

If the diarrhœa be associated with vomiting, this should be encouraged and assisted by copious draughts of tepid water. The vomiting affords relief partly by the stimulus which it gives to the circulation, but mainly by the speedy ejection of morbid secretions.

If there be nausea without vomiting, and more especially if the stomach be supposed to contain undigested or unwholesome food or morbid secretions, an emetic may be given—either a teaspoonful of powdered mustard or a tablespoonful of common salt, or twenty grains of ipecacuanha powder in warm water.

When vomiting is excessive in violence or in frequency, it may sometimes be checked by small draughts of iced water at short intervals, or by lumps of ice taken and allowed to melt in the mouth. Another plan is to place three grains of calomel on the tongue and wash it down with cold water. The calomel probably acts by reversing the action of the stomach and bowels, and thus causing evacuation downwards. When given as a powder it

is less likely to be rejected by the act of vomiting than when made into a pill.

Thirst may be allayed by drinking cold water, which may be acidulated by the addition of lemon-juice or a few drops of aromatic sulphuric acid. *Care should be taken that the water for drinking is pure.* Organic impurities, such as result from the admixture of sewage, are especially to be dreaded. If the water be of doubtful purity, it should be carefully filtered through sand and charcoal, and then boiled. Impure water is a common exciting cause of cholera.

While the diarrhœa continues, the diet should consist mainly of milk, rice, arrowroot, gruel, broth, or beef-tea.

In all cases of severe diarrhœa the patient should remain in bed.

If, while the purging continues, the stools become colourless and watery (the purging being of the kind commonly called rice-water purging), or if, without a continuance of the purging, the surface of the body become cold and blue, the disease is now passing, or has actually passed, into the stage of collapse.

In the worst forms of collapse, as cholera presents itself often at the outbreak of an epidemic, the disease is so deadly that no treatment is of any avail; and a sure way to bring discredit upon any method of treatment is to apply it in this desperate class of cases.

Choleraic collapse, as I have before explained, results from a peculiar arrest of the flow of blood through the lungs, occasioned by a morbid poison. It is not a condition of mere exhaustion. It is not relieved by the remedies for exhaustion; and it is made worse by opiates and by spirituous stimulants, which must therefore be avoided. The injurious influence of opium and alcohol in the collapse stage is now almost universally admitted. Their noxious effects are intelligible, if we bear in mind that during the collapse the oxidation of the blood and tissues is greatly diminished, and that both opium and alcohol still further impede oxidation and thus add to the peril, while opium also retards the escape of the poison.

The patient should be strictly kept in the recumbent position; *he must not be raised even to go to stool*. He should be abundantly supplied with fresh air, and should be allowed to drink pure water freely. The water may usually be taken *cold*, but it should not be *iced*, except occasionally when given to check excessive vomiting. Large quantities of iced water may do harm, by chilling the patient and checking the natural eliminative efforts. In cases of extreme collapse I would persuade the patient to drink *hot water*, for the purpose of warming the blood and quickening the circulation. Some care is required not to over-distend the stomach by liquid. Unless vomiting occurs from time to time, the drinking of large quantities of liquid may so distend the stomach

as to impede the breathing, and thus cause much distress. The circulation has sometimes been improved by the injection of large quantities of hot water into the rectum.

Hot flannels, or bottles, or bags of sand, should be applied to the feet and legs.

Cramps in the muscles are, as a rule, more frequent and severe during the diarrhoea stage than during the stage of collapse. Whenever they occur, they may be relieved by rubbing the affected parts with the warm hand. There can be no objection to the use of anodyne or stimulating embrocations, but they are of doubtful advantage. Cramps in the legs may often be relieved by enveloping the limbs in flannel wrung out of hot water, with a covering of macintosh or oiled silk.

Hot baths, whether of water or of air, although they relieve the painful cramps and often effect a temporary improvement of the circulation, have been found to be, on the whole, more distressing and exhausting than beneficial.

Five grains of sesquicarbonate of ammonia, or a teaspoonful of spirit of sal volatile, may be given in an ounce of camphor mixture every two or three hours as a diffusible stimulant.

The discharges from the bowels, and the condition of the abdomen, should be carefully observed. In favourable cases the discharges always continue, more or less, during the stage of collapse and until reaction has set in. One of the earliest and surest

signs of reaction is the reappearance of bile in the vomited matters and in the stools. When vomiting and purging entirely cease during the stage of collapse, the disease is nearly always fatal.

One of the main objects of treatment during this stage is to facilitate the escape of the morbid secretions from the alimentary canal. This may be done partly by the copious use of diluent drinks, and partly by an occasional dose of castor oil. If we carefully observe the condition of a patient in collapse, we shall often find that the intestines are more or less distended with liquid, and this, too, while perhaps there is general torpor and little or no effort at expulsion, sometimes the result of opium, but it may be due to the defective oxygenation during collapse. Again, it has often been found that, although there has been copious watery purging during life, the small intestines contain after death a large amount of a peculiar viscid dirty white material, having a very offensive odour. An occasional dose of castor oil may be useful in removing both these conditions: namely, over-distension of the bowel by liquid, and accumulation and retention of offensive viscid semi-solid secretions.

I have no doubt that by this treatment I have rescued patients who would have died if they had been left to the unaided efforts of nature. I can lay down no rule as to the number of doses of oil that may be required. It should be borne in mind that the object and the effect of the evacuant or

cleansing treatment are not to increase the amount of liquid which is poured from the blood into the stomach and bowels, but simply to assist and to quicken the expulsion of the morbid secretions from the alimentary canal.

It may be confidently maintained that, inasmuch as the peculiar choleraic discharges are the result of specific blood-changes, induced by a morbid poison, no ordinary purgative can *increase* those discharges, although it may facilitate and quicken their expulsion.

The volume of liquid which is discharged from the stomach and bowel, is, *in the diarrhœa stage*, a measure of the dose or the virulence of the poison, just as, in the case of diabetes, the flux of urine is equivalent to the amount of sugar that has to be discharged through the kidneys. During *the stage of collapse* the liquid discharges cease to be an index of the severity of the disease, for the reason that the partial arrest of the circulation impedes, more or less, the excretion of the poison and its products through the alimentary canal, and in the same degree increases the risk of a fatal result.

Bleeding from the bowel sometimes occurs. It is a very unfavourable and usually a fatal symptom. The best treatment consists in giving twenty drops of oil of turpentine in mucilage every two hours, and iced water may be taken freely. *In such a case no castor oil should be given.*

After reaction has occurred, an occasional laxa-

tive dose is required—about once in the twenty-four hours during the first two or three days.

It is worse than useless to attempt to *feed* a patient during collapse. The secretions of the stomach are utterly deranged; and the power of digestion is suspended. The mildest nourishment administered at this time only adds to the feeling of oppression and general distress, from which the act of vomiting often gives immediate relief.

After reaction has occurred, and when the normal secretions are restored, the mildest nourishment should be given frequently, but in small quantities—such as milk, gruel, or rice, or arrowroot with a small quantity of brandy, soup or beef-tea, or chicken-broth. After an attack of cholera the stomach is sometimes long in recovering its tone and the power to digest solid food. When this is the case, a grain of quinine, with ten or fifteen drops of dilute hydrochloric or sulphuric acid and an equal quantity of spirit of chloroform, may be taken with each meal.

Venesection has often afforded great relief during the stage of collapse. The case recorded by Sir Ranald Martin affords a striking illustration of this (see p. 10). Many similar cases are recorded in the writings of practitioners in India. The symptoms which appear especially to call for venesection are rapid breathing, and a feeling of impending suffocation. When, with these symptoms, there is a cessation of vomiting and purging, which is probably a

result of the almost entire arrest of the circulation through the lungs, venesection may prove a life-saving remedy. It is difficult to obtain a stream of blood in these cases; not, as many suppose, because the blood is too thick to flow, but because, in consequence of the block in the lungs, the blood in the veins is nearly stagnant. The bleeding appears to be beneficial chiefly by lessening the distension of the right cavities of the heart, and so increasing their contractile power. Repeated doses of ammonia may help to quicken the circulation, and so favour the flow of blood.

Hot saline injections into the veins have unquestionably saved life in some apparently desperate cases. This is an operation the performance of which requires great skill and care. I have already explained its mode of action (p. 10).

Consecutive Fever.—Reaction from collapse is frequently followed by a febrile condition—a hot skin, quick pulse, coated tongue, hurried breathing, often a scanty secretion or even a complete suppression of urine, with drowsiness tending to pass into coma. These unfavourable symptoms are more common when, during the earlier stages of the disease, opium and alcoholic stimulants have been freely given; but they may occur when no such means have been employed.

The best treatment consists in a scanty liquid diet *without alcohol*, copious diluent drinks, with saline effervescing draughts, an occasional aperient, castor

oil, or sulphate of magnesia or soda or a seidlitz powder, counter-irritation over the lungs and kidneys, and sometimes local bleeding by leeches or cupping to relieve congestion of those organs.

When the urine is suppressed or scanty, one of the safest and most efficacious diuretics is the "Imperial Drink," made as follows: A quarter of an ounce of cream of tartar, the juice of one lemon, the outer peel of half a lemon, a pint of boiling water and sugar *ad libitum*. One or two pints of this may be taken cold in the course of the twenty-four hours. Alcoholic stimulants should be withheld until the function of the kidneys has been completely restored.

It will be seen that the treatment which I recommend, and which has been practised by myself and others with great success, is not a system of mere drug-giving, but a definite plan based upon a careful study of the natural history of the disease. I am confident that any one who, having an intelligent appreciation of the pathological conditions to be dealt with, will carry out this method of treatment with the same determination to arrive at a definite result as that with which he would treat a case of poisoning by opium, will soon have better reasons for continuing the practice than any with which I can furnish him. For the full success of the treatment it is necessary that the practitioner himself should have that confidence in the method without which it can never be fairly tried, and that he should

inspire his assistants, and above all his patients, with hope and confidence.

Preventive Measures.—The choleraic discharges from the bowels should be looked on as highly poisonous, and they should be disinfected and got rid of as soon as possible. Every vessel and article of clothing or bedding soiled by the discharges should be carefully cleansed and disinfected. The attendants on the sick should be warned of the necessity for thorough ventilation and for extreme personal cleanliness. The hands should be frequently cleansed with the aid of disinfectants, and always immediately before taking food.

If these simple measures are adopted, nurses and other attendants on the sick run little risk from infection,

The chief disinfectants are—chloride of lime, Burnett's liquid, Condyl's fluid, and Calvert's solution of carbolic acid. These disinfectants are sold with full printed directions for the use of each. Diluted Condyl's fluid is well adapted for cleansing the mouth and hands before taking food; and carbolic acid for cleansing bedding and clothing, which would be damaged by mineral disinfectants. Any one of the above may be used for disinfecting the stools.

A concentrated solution of sulphate of iron is also a cheap and efficient disinfectant for the discharges.

The meals should be taken at regular and not

too great intervals. Long fasting and over-fatigue should be carefully avoided.

Great moderation both in food and in drink is essential for safety during an epidemic of cholera. A single act of indiscretion has been followed by a severe attack. Intemperance at such a time is fraught with extreme danger.

Unwholesome articles of food, more especially tainted meat and fish and decayed vegetables, are to be carefully avoided. It is a mistake to suppose that entire abstinence from fruit and vegetables is necessary or wholesome during a cholera season. Ripe fruit and fresh vegetables may be taken in moderation with safety and advantage.

Especial attention should be paid to ensure the cleanliness and thorough ventilation of dwelling-houses. All vegetable and animal refuse should be removed as speedily as possible. Care should be taken to prevent the escape of sewer poison into the interior of dwellings.

The purity of the water employed for drinking and cooking should be most carefully provided for. A few drops of Condyl's fluid may be used as a test for the purity of water. Organic impurities in the course of an hour or two decolorise the fluid ; which is not only a test, but also a purifying agent by oxidising the organic impurities. It is certain that a diarrhoea is often perpetuated by the daily drinking of impure water ; and it is equally certain that to arrest a diarrhoea having such an origin by

opiates and astringents is a most dangerous practice. I have seen instances in which a diarrhoea, probably caused by drinking foul water, having been arrested by opium, was quickly followed by an attack of cholera of the most formidable character.

No unnecessary medicines of any kind should be taken. When opening medicine is required, the mildest should be selected, such as castor oil or rhubarb. Saline purgatives, such as Glauber's salts and Epsom salts, are objectionable, on account of their tendency to cause profuse watery purging. On the other hand, the common belief that prolonged costiveness should not be interfered with during the prevalence of cholera is an error. An accumulation of offensive materials within the bowels may be itself a source of irritation and of danger.

Some practitioners believe and assert that cholera collapse may be caused by a dose of purgative medicine. This I entirely disbelieve. It must have happened frequently that an individual, experiencing the *malaise* which results from the cholera poison in the system, takes a dose of opening medicine, the symptoms of cholera develop themselves, and the disease may be supposed to have been caused by the medicine; yet such a conclusion would be as inconsistent with facts and with analogy as to attribute the outcoming of the rash of scarlatina to a mustard poultice applied to the

neck for the relief of the sore throat which commonly marks the commencement of that disease. Cholera and scarlatina are both specific contagious diseases; one could no more be caused by a purgative than the other by a mustard poultice.

It is important to know that cholera may be imported into a district by a person coming from an infected locality while he is suffering from diarrhœa. The diarrhœa may be a mild form of cholera, and the discharges will then contain the specific poison of the disease. If these discharges are permitted to contaminate the soil, the water and the air, the disease may spread with destructive rapidity. To prevent an outbreak of cholera, the diarrhœa patient should, if possible, be kept in quarantine, the discharges should be disinfected, and then buried, and all soiled clothing and bedding should be carefully disinfected and cleansed or burnt.

Public latrines in a district where cholera prevails are highly dangerous and a fruitful source of infection. The use of latrines by British troops in India has often led to a great mortality, while the native troops, who never resort to latrines, have either escaped entirely or have suffered in a much less degree, at the same time and place.

The early burial of those who have died from cholera is imperative as a sanitary measure, more especially in a tropical climate, where decomposition is extremely rapid. Of course care must be taken to ascertain that life is really extinct before burial takes

place. And here it may be well to mention that there are two post-mortem phenomena which the inexperienced might mistake for signs of vitality. The internal heat of the body, which even during collapse is usually above normal, comes to the surface after death, so that for an hour or two the temperature of the skin may be higher than it was for some hours before death. Again, it not infrequently happens that for half an hour or so after death some muscles, probably stimulated by the poisoned blood which excited the painful cramps during life, are affected by irregular contractions, which may be sufficiently powerful and concerted to move a finger and even a limb. These purely physical post-mortem movements have often excited a vague terror, and a suspicion that life remains in the moving corpse.

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